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The Hon. Ed.c Everett &c

ROYAL SOCIETY.

CORRESPONDENCE OF THE COMMITTEE ON THE SOUTHERN TELESCOPE.

PRINTED BY ORDER OF THE PRESIDENT.

Mr. Nasmyth to the Earl of Rosse.

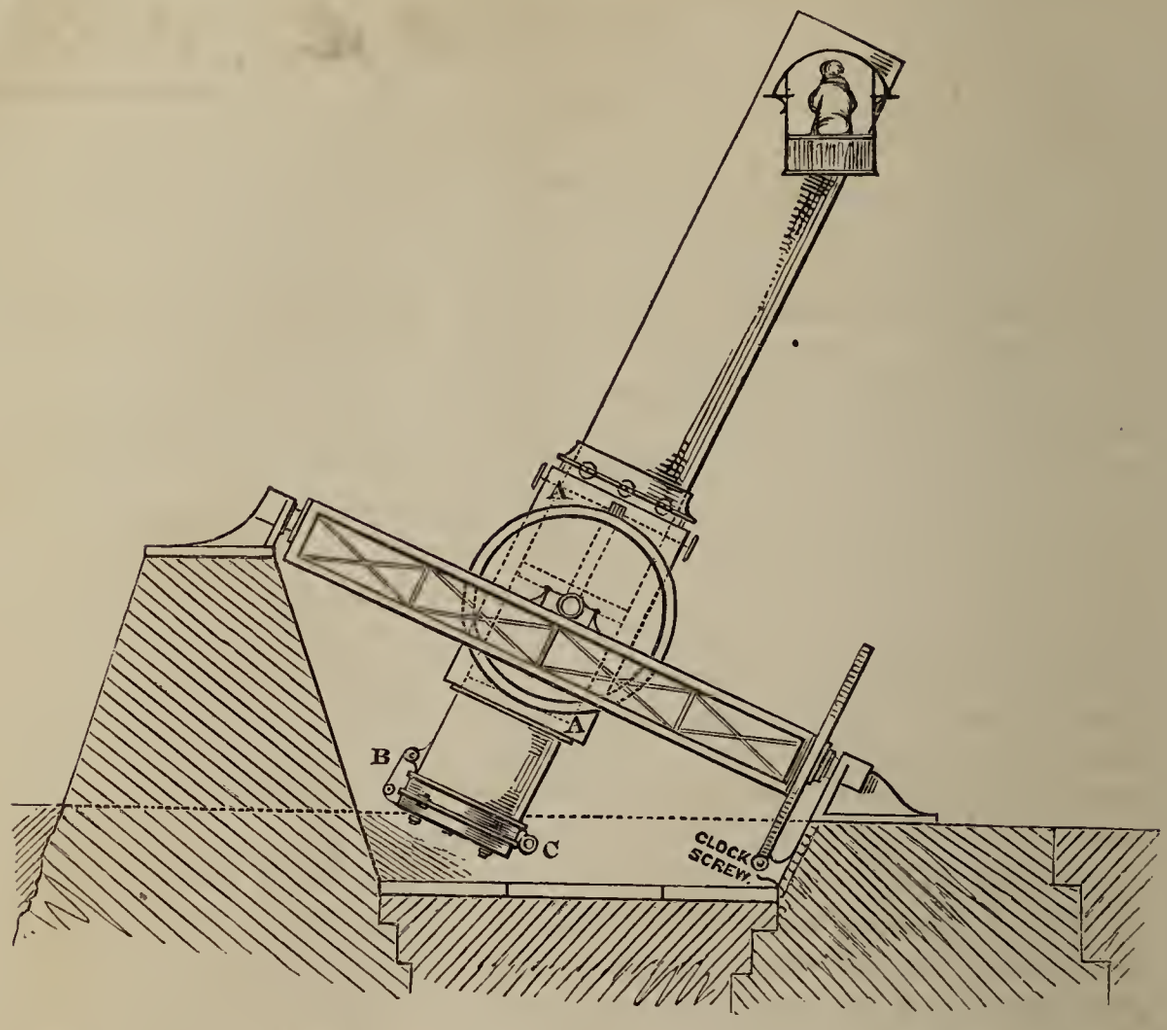
Bridgewater Foundry, Patricroft,
near Manchester, Dec. 15, 1852.

MY LORD,

With respect to the system of mounting such an instrument as is proposed, I should vastly prefer the equatorial, and do not think that even in respect to expense it would prove so much more costly than the altitude and azimuth, and decidedly superior as to general steadiness and ease of management, and for securing by clock-work movement perfect tranquillity to the observer, who I would propose to place in a very snug box or box-seat slung to the eye-piece socket. After selecting his object he would have nothing to do but sit at his work, carried along with the instrument, and the object ever in the field. As the powers requisite to use up all the light of a four-foot metal must be seldom much under 360 to 400, were we to attempt to follow the object by other than equatorial clock motion, I fear there would be little of that tranquillity so requisite in making careful observations and sketches with so powerful an instrument.

The following rude sketch is somewhat like the way in which it appears to me such an instrument should be mounted. The polar axis is a strong frame of cast iron, between the sides of which the telescope-tube swings; the observer is in his snug box, slung to the eye-piece socket, so as to sit always on a horizontal seat whatever may be the position of the instrument, the seat of his box being universal for that object; the tube to revolve in the square socket bore A, so as to give means of correcting "the screw" of the tube consequent on the equatorial arrangement, and in that way keep eye-piece, observer and metal always in one constant or nearly constant parallel. I would recommend my ? plan of having the metal in a hinged cast-iron cell, so that by a small windlass at B you can let back the cell

on the hinge C, and get in to wipe it, or cover it with the utmost care. I find this a most handy system.



With sufficient mass of material in the frame or polar axis, I should not fear any want of steadiness. Even in such a windy night as would be at all fitted for observation, the observer might close his shutters and be as snug as at his own parlour fireside; for when once he had sat down to his work on the object then in hand, with the clockwork in gear, he need not trouble his attendant for the next three hours, if he select such objects as are near the meridian. With respect to the cost of such an instrument, I have made a rough estimate of what appears to me would be a fair price for so out-of-the-way and anxious a job, including a suitable polishing machine, which I conceive to be absolutely requisite. I should say the maker, whoever he may be, ought to have £3000 at the least. I include in this two metals of not less than 6 inches thick, ground on the back to the same curve as the face, supported of course on your Lordship's admirable plan of, say, 81 self-adjusting points or friction balls. I would fain have had the focal length equal to 9, or at least 8, diameters of the metal.

If we could prevail upon Messrs. Maudsley to undertake this work, I think we could supply them with all the information as to casting, grinding and polishing which they would stand in need of.

(Signed) James Nasmyth.

To the Right Honourable
The Earl of Rosse, &c. &c.

The Earl of Rosse to Colonel Sabine.

Castle, Parsonstown, December 18th, 1852.

DEAR SABINE,

The equatorial mounting as proposed by Nasmyth would be decidedly preferable to the altitude and azimuth mounting of our three-feet telescope, provided it was steady. The worst consequence to be apprehended, should there be a want of steadiness, is *impaired definition*. Of course with an unsteady instrument, micrometer-work would be very unsatisfactory, but that *comparatively* is a subordinate consideration. Nasmyth's mounting is in principle what has been called the *English mounting*, a long polar axis being employed instead of a short bearing. So far as I am aware, no large instrument so constructed is steady except the Liverpool equatorial. In the case of the Liverpool equatorial the weight of the telescope bears a very small proportion to the weight of the polar axis, and that, in my opinion, is the reason why it is steady. I am speaking merely by guess, but I should think the weight of the telescope is probably not more than $\frac{1}{16}$th the weight of the polar axis. On this point, and I think it a very important one, precise information could easily be obtained. The equatorial here is on the English plan, and is, I believe, the largest instrument of the kind. It is not perfectly steady, and I am sure it would not work satisfactorily in the open air. The speculum is 18 inches aperture, and 10 feet focus; the polar axis is 14 feet long, and $3\frac{1}{2}$ feet largest diameter, made of wood like a cask. The staves are very strong, and were impregnated with sulphate of copper; then baked, and closely hooped. After hooping, the whole was baked, and the hoops re-driven. The ends are of oak, in alternate layers 12 inches thick, glued and bolted together, to which the staves are attached by thirty dozen of screws, aided by glue. The whole was covered with a cement of bees'-wax resin, whiting, and spirit of turpentine to protect it from hygrometric action. The tube of the declination axis is contained in a copper pipe, which makes air-tight joints with the polar axis, so that damp air is excluded. The polar axis was turned to receive its fittings, and so well has it kept its figure that it now runs as true in its bearings as it ever did. Its stiffness is so great, that a small telescope, magnifying about fifteen times, having been fixed to one extremity, and looking at a fine mark fixed at the other extremity, (so as to test flexure) when the polar axis with its full load was turned round 180°, the flexure was imperceptible. With this great strength, however, the torsion is sufficient to produce considerable unsteadiness in right ascension in handling the instrument. The declination axis, on the other hand, is but $3\frac{1}{2}$ inches thick at the bearing close to the telescope, and yet no instrument can be freer from tremor in declination. In the one case the telescope is 7 feet from each bearing, and in spite of enormous strength we have torsion and tremor; in the other case it is scarcely 3 inches, and there is no perceptible tremor or torsion, though the axis is comparatively feeble. The weight of the telescope may be to the weight of the polar axis as about 1 to 4. In Mr.

Nasmyth's mounting the telescope can scarcely be less than 12 feet from each bearing; and in my opinion nothing but *enormous mass* can prevent torsion, and so much unsteadiness, as will render the instrument of little value in the open air. It will be necessary to form at least some rude conception of what the mass should be; unfortunately in these motions we cannot safely trust to mere dynamical considerations; there is much which is very obscure, and we must, as it were, feel our way. As the size of instrument increases, the proportionate strength of materials diminishes; it would not therefore be safe to infer that a polar axis sixteen times heavier than the telescope would be sufficient. There may, however, be an excess of strength in the Liverpool polar axis, and probably this question might be tested by loading the telescope, if the telescope is strong enough to bear this rough treatment without injury. Suppose the same proportions as the Liverpool equatorial should be considered safe, which I have assumed to be as 1:16: roughly speaking, the weight of the telescope may be somewhat as follows :—

	Tons.
Speculum....	$1\frac{1}{2}$
Box and levers	$\frac{1}{2}$
Tube.....	4
	6
	16
	96 tons, weight of polar axis.

This at £20 per ton, including patterns and millwright's work, would be £1920 as the cost of the polar axis. The brass circle and clock would, I should think, be at least £200 or £300 more; the tube and fittings perhaps £500, and the two specula £1000. The polishing machinery with a one-horse steam-engine to drive it, would cost perhaps £300.

£1920
300
500
1000
300
£4020

There should be an unusually large margin for contingences, so that, I think, it would be scarcely safe to estimate the cost at less than £6000.

Were it determined to execute such an instrument, it would be highly desirable that a good working model, furnished with an excellent telescope, should be made previously, and thoroughly tested. The telescope itself should also, in my opinion, be put up near the maker's in a temporary way, and well-tried at *real work* before it was sent out. In the event of failure the tube would answer for an

altitude and azimuth mounting. An instrument mounted like my three-feet telescope would cost, I think, about one-half. The equatorial mounting, if really successful, is so much better than my plain stand, that if the Government could be prevailed upon to incur the expense the experiment certainly ought to be tried. I have just received Mr. Smyth's ingenious plan for mounting a large telescope equatorially, but have not considered it with the attention it deserves. I fear the cost would be enormous, and I scarcely think the mass could be safely made materially less than in Nasmyth's. The distance between the pivots of the polar axis, and the point to which the driving force would be applied, is so considerable, that there would be a strain which the form would not be well calculated to resist; and where there is much torsion I fear it would be difficult to control the tremors to which it gives rise. Should the Messrs. Maudsley be disposed to undertake the instrument, Mr. De la Rue could I am sure assist them in the polishing, and when in London I should, of course, be very happy to afford them any information in my power. Extreme pressure of business of different kinds has prevented me from writing to you as soon as I intended. I should have been glad to try the experiment, whether the movements of the observer, placed as proposed by Mr. Nasmyth, would be likely to disturb the instrument, but the weather has rendered it impossible.

(Signed) Rosse.

Postscript.

I omitted to add to my letter of yesterday that I think the German mounting is preferable to Nasmyth's, and to every other which has been suggested. It has its disadvantages, its small circles being objectionable; still, looking to the two great questions, steadiness and expense, I think it has greatly the advantage. I think it would be safer to speak with confidence about it than any other. I have not seen Mr. Cooper's, but I believe from what I have heard it is perfectly steady, and the telescope is unusually heavy for its length and aperture. It is used in the open air, with however some shelter from a wall. Mr. Cooper's is the German mounting, and I think it might in some measure be taken as a large working model.

Very truly, &c.,

Rosse.

Sir D. Brewster to Mr. Weld.

St. Leonard's College, St. Andrews, Dec. 20th, 1852.

Dear Sir,

I may mention to you, what I thought it right to state to Lord Wrottesley, that when I was on a visit to Lord John Russell last autumn, I was describing to him Lord Rosse's great telescope: he, of his own accord, said, "Would it not be a right thing to send a large telescope to a southern climate?" I replied, "Certainly; but your Lordship would not give us money to do it;" and I added, "that I felt the disappointment particularly, as I was President of

the British Association when it was refused." Lord John said that he had no recollection of the matter; and I stated in a way not to be misunderstood, what I firmly believe is the truth, that some interested person had interposed on the occasion and prejudiced the Government against the scheme.

If another application is made to the Government, which, of course, it will be, I trust that the Committee will take care that it is made by persons *whose hearts are really in the cause*, and that they will take care to meet any underhand opposition that may be made to it.

I consider Prof. Smyth as pre-eminently qualified to take charge of the new establishment, and I look forward to results of the highest kind from his superintendence.

I am, dear Sir,
Ever most truly yours,
D. Brewster.

C. R. Weld, Esq.,
Assist. Sec.

Mr. C. P. Smyth to Capt. Smyth.

London, December 22, 1852.

My dear Father,

Finding that there is a greater length of time to spare than had been expected before the next Meeting of the Southern Telescope Committee, I take advantage of the opportunity to write more fully on several of the topics which are to be considered. It may lead to a simplification of the question if I withdraw from the conditions mentioned in your letter of December 7th to Mr. Bell the passage to the effect, "that the new establishment was not to be inferior to that of the Cape." There then only remain,—

1. The mounting of the Telescope.
2. The site of the Observatory.
3. The publication of the results.

The mounting of the Telescope.—The telescope is, I believe, to be a Newtonian reflector, with an aperture of 4 and a focal length of 30 feet. Its weight therefore will be very great, and a mounting of the altitude and azimuth construction would be the simplest mechanical solution of the problem of enabling it to be pointed to every part of the sky.

But in the present case, where not only eye-views of the nebulæ are required, but accurate numerical measures of them also are indispensable, and should form the leading feature of the establishment, the simple altitude and azimuth-stand will not be found sufficient, without at least the assistance of some additional directive apparatus for equatorial motion, such as that devised by Mr. Airy for some temporary stands at Greenwich

Were there however sufficient truth of movement obtained by these means, and in a sufficiently simple manner for the purpose of micrometrical measures, it would be found still more difficult to keep up the motion with that perfect uniformity through a long period of time, which would be absolutely necessary in the applica-

tion of photographical processes to picturing the field of view. This is a method too which it is of the highest importance to introduce, and which, though at present applicable to but a limited range of objects, will without doubt be continually undergoing important extension.

The very strict requirements of this latter mode of procuring results would seem to oblige us to adopt the parallactic form of mounting with clock-work motion. In principle nothing can be better; and in practice it has proved, in various instances, all that could be wished. Still it is apprehended that there would be great difficulty in applying it to so heavy an instrument as a 4-foot speculum.

On looking around we certainly find that no equatorial mounting strong enough for such a reflector has yet appeared, but that is no proof that it is impossible. Often by discovering errors of principle in the older constructions, or by employing stiffer materials and greater mass, has the principle been found capable of extension to larger sizes. And the recent immense advances in the manufacture and the employment of both cast and wrought iron, may be taken as an assurance that a far more powerful parallactic instrument may now be produced than has ever yet been seen; and I will enclose a plan of my own, by which I have strong hopes that a mounting of almost any size and strength could be formed.

The site for the Observatory.—In addition to all the facilities for producing good observations, afforded by the space-penetrating power of a telescope, as well as by the style of its mounting, much must always depend on the latitude of the station selected, and more still on its elevation above the level of the sea.

This arises mainly from the disturbing effects, always present in a greater or less degree, of the atmosphere, increasing too with the aperture of the telescope, as well as with the magnifying power employed. But by simply mounting up 1000 feet above the level of the sea, the quantity of the atmosphere to be looked through, in a zenith observation, will be diminished by so large a portion as $\frac{1}{30}$th of the whole.

Thus there were easily observed with Bradley's zenith sector on the Cape Mountains (2000–5000 feet high) certain small stars, of which nothing could afterwards be made at the Cape Observatory; while with larger stars, seen at both stations, the mountain-observations were more accordant, and in so far better than the others, owing apparently to the momentary fluctuations of refraction decreasing in proportion to the rarity of the air.

Elevation is also of importance in raising the observer above the *mechanical* impurities of the atmosphere, such as fine dust, smoke, and the vaporized particles of solid bodies, well known under the familiar term of motes in the sunbeam. These, rather than the gaseous constituents, are what produce the general diffusion of light over the sky impeding photographic action, and, if abundant (according to the experience of a friend in South Africa) preventing the attainment of the highest polish on specula.

If completely immersed in this dust, we are not conscious of the

extent of its effect; but on rising up the side of a mountain, even on reputedly clear days, we then plainly see the impure medium below. All objects within its reach are then observed to have their edges indistinct and thin, the dark and bright parts confused together; while the sharpness of the outlines, the pungency of the light, and the transparency of the shadows of anything above its range, form a most striking contrast.

When a star by night, or a heliotrope by day, was seen through this dusty matter at the Cape, it was invariably blurred into a diffuse sort of nebula, and the space-penetrating power of its light was greatly diminished, while nebulæ proper were rendered invisible. And if the medium was of more than ordinary density, it was found to prevent even the largest heliotropes being seen at short distances, though their light would otherwise penetrate with facility through far darker vapour when of the watery kind, and through actual rain, neither of which seem to have any prejudicial effect on definition; indeed, the only good observing nights at the Cape were when dew was falling.

From the very general distribution of this finely divided matter, the lowest stratum of the atmosphere, for a thickness of about 5000 feet on the average, may be regarded as a dry and dust-bearing region; above which commences a moist and cloud-bearing one, and rises to a greater height than has yet been reached by man. From the result of two years' experience, on the S. African mountains, both a daily and an annual variation in quantity are manifested, the dust being more abundant in the afternoon than in the forenoon, and in the autumn than in the spring; or in much the same ratio in which photographers experience a retardation of the chemical action of the solar rays, owing very probably to the yellowish colour transmitted by the particles.

They appear moreover to be confined as to their range of altitude in the summer to 3000 feet, while in the winter they often rise above 7000 feet—an effect, Professor Stokes has suggested, of the *viscidity* of the gases composing the atmosphere and bearing the particles of matter, combined with the effect of heat on that quality.

From this partial sinking of the dust in summer there results a concentration of it near the surface of the earth, in proportion to the warmth of the country and the season. Hence perhaps the remarkable fact that the chemical action of the sun's rays is much weaker there, notwithstanding the superabundance of light, than it is in the temperate and less illumined zone. This circumstance, while it increases our difficulties at the level of the sea, relieves us of them more and more completely on elevated situations, in proportion as the heat is greater, which, conjoined to the more bearable temperature for the observer, points out the advantage of employing an *elevated* situation in the *tropics* for the observation of nebulæ.

Of the three plans already mentioned at the Committee, viz.

Cape Town in lat. S.	34°
Sidney	34
Hobartown	42

none suit the above conditions very well, either as regards latitude or elevation, for they are none of them sensibly elevated above the level of the sea, and it would be necessary at least to abandon the towns and seek more appropriate sites in the adjoining country.

But even with this modification nothing very favourable can be elicited; for at the Cape the best position afforded is the Khamisberg, with an inhabited summit 5000 feet high. But it is almost inaccessibly beyond broad, difficult tracts of desert land, and is almost entirely deficient of building-materials and labourers. Australia is put out of the competition by the effects of the gold discovery; and in Van Diemen's Land, though some circumstances are more favourable, as the abundance of wood, of convict labour, and of good public roads, yet the highest available table-land there seems to be not much more than 2000 feet high.

If therefore the utmost advantage is to be taken of natural circumstances to second the optical power of the telescope, recourse must be had to other countries, and of all in the Southern hemisphere, the elevated plains of the Andes would appear to be the most favourable, and might be adopted but for political difficulties. These, however, would fortunately offer no bar to employing the new Sanatorium of Ceylon, where the height of at least 6000 feet could be gained.

Something might thus be lost on the three first stations as regards latitude for Southern objects, but much would be gained in altitude; and if photography is to be applied to the registration of telescopic phenomena, every other consideration should give way before this most important one of elevation of site, and for this reason.

The atmosphere is never in so perfect a state, that a highly magnified image in a telescope appears perfectly quiescent; it is always fluctuating more or less with a variety of motions and alterations of shape. In ordinary observing the eye follows all these movements, and is able to acquire the same exact idea of the star as it may of the masts and rigging of a vessel that is slowly heaving at sea, though such motion during the formation of a camera-photograph would prevent anything appearing beyond a blurred mass in the mean position of the hull for the time. As many of the optical tremors too of the atmosphere are so rapid as to be repeated many times in a second, no improvement in photographical processes, short of perfect instantaneity, can remove this serious practical difficulty. And the only mode by which we can at least diminish it, is by decreasing, through elevation of site, the quantity of the atmosphere through which we look.

The publication of the results.—The establishment of any observatory by Government being for the benefit of science at large, the speedy publication of the observations at stated intervals, with all the needful calculations and reductions, should be considered as an absolute requirement; and more particularly is it the case in the instance now contemplated, as it would form the least exceptionable, and perhaps the only possible substitute for "visitation" and supervision.

The numerical results might indeed be sent home from abroad, and be printed by deputy; but this cannot be done with the drawings, which will necessarily form a large part of the "results," and no one but the artist himself can efficiently revise an engraving after one of his own drawings.

Either therefore the astronomer must come to England occasionally, and see his works through the press, or he must have the engraver with him at the observatory, and the plate-printer too, for there is no good engraving to be performed without frequent proofs being pulled in the course of the work; and as sufficiently good engravers and printers do not exist at present in the Southern hemisphere, they would have to be sent out expressly for this purpose.

In place of engraving, we may, however, now take advantage of the improved photography of the present day on glass-plates, as a method of multiplying drawings with abundant accuracy and sufficient artistical perfection.

Photography may be dearer than mechanical printing; but considering that only a limited number of copies will be required, and that the whole expense of engraving will be saved, it may be found a saving on the whole.

I remain,
Your ever, &c.,
C. Piazzi Smyth.

Capt. W. H. Smyth, R.N., F.R.S.

Appendix.

Note on a Method of Constructing an Equatorial Mounting for a large Reflector. *By* C. Piazzi Smyth.

When the focal length of the telescope is short, the stand should be of cast metal, and ought to be placed under a revolving roof. But if a greater length be decided on, the dome may be dispensed with; for, according to the experience of Lord Rosse, the wind does not shake the larger of his several telescopes in a more rapid proportion than the surface, probably on account of the weight increasing with their size.

The observer himself, however, should be protected from the weather, as no accurate measurements can be taken by any one exposed immediately to the tempestuous gales which blow with clear skies at the Cape and other places in similar latitudes. The telescope tube moreover should be fastened at both ends, as in the altitude and azimuth stands at Birr Castle, so that the wind may have no leverage over the fixings.

Then by adopting a construction of wrought-iron plates, to be put together on the spot with red-hot rivets, as in the case of the Britannia tubular-bridge, a parallactic mounting for a telescope of any size, say 100 feet long, may be made in the following manner, and with the full preservation of all the above-mentioned conditions.

1st. Let the fundamental portion of the polar axis be a wheel

100 feet in diameter, with a very strong hollow rim made of plate-iron, 1 inch thick, with radial stops, and 7 feet broad by 15 deep (in the plane of the wheel). Four strong spokes, made also of wrought plates, to carry a cast and turned ring 10 feet in diameter, as the nave of this wheel, which will be also employed as the declination circle. The polar-axis pivots to be tubular, 3 feet in diameter, and to be fastened on the rim of the wheel by broad bases, the whole being bound together by tires of malleable iron put on red-hot.

2nd. To strengthen the above laterally, as well as to afford the means for clamping and giving slow motion in right ascension, let there be fixed to the back of it an "equatorial semicircle," of the same tubular construction and with the same radius, and let the driving clock act upon its periphery.

3rd. For further strength in inclined positions let there be two "six-hour quadrants," connecting the middle of the equatorial semicircle with the rim of the declination wheel at the polar-axis pivot points.

4th. The declination axis to be a conical tube 3 feet in diameter at one end, and 10 feet at the other, with a broad outward flange in addition, and long enough to reach from the outward face of the declination circle, through the whole structure, to the junction of the equatorial semicircle and the six-hour quadrants, there to be held in by a powerful screw.

5th. The telescope tube to be double, and of $\frac{1}{2}$ inch wrought iron plate, with longitudinal ribs riveted between.

6th. This cylindrical tube to be enclosed within a prismoidal one, by one whose flat surfaces may be attached to the flange of the declination-axis. The angular corners of the outer tube to be fitted at short intervals in its length with stops, so arranged with large central apertures as to form enclosed ladders, whereby the observer may climb up from the speculum end of the telescope to the eye-piece end. There the prismoidal tube may be expanded into a small room, which will screen the observer from the weather, and may be so ventilated up one side of the casing between the tubes, and down the other, that no heated air shall pass over the mouth of the telescope.

7th. The face of the declination circle will bear graduated arcs of turned metal for pointing the telescope by; and a rack edge, to be worked in by a pinion, which may be turned for the observing room, to give motion in declination.

8th. An arc of turned metal, with a toothed edge, is also to be fastened to the rim of the equatorial semicircle for the driving clock to act upon; and this being elevated on a pier to the level of the centre of the polar axis, and placed east or west so as to act at six hours' distance from the meridian, allows of an uninterrupted motion in every direction.

This new form of equatorial will thus be firm and stiff in every direction; its long polar axis and small terminal pivots at either end will ensure great truth of movement in right ascension. The driving clock will act with ease on so large an hour-circle, whose plane too,

passing through the bearing centre of the telescope, reduces the length of the polar axis, affected by torsion, to a zero. The telescope, moreover, is clamped at both ends, and to as firm a declination circle as could be desired; while the protection and security given to the observer, with the power of directing his instrument without descending from his post, the ease and certainty of setting to the faintest objects, and the command of the whole sky, uninterrupted by any mechanical difficulties of reversal on passing the meridian, or otherwise, render this parallactic mounting as convenient for use as it is safe and trustworthy, on account of its strength and its composition wholly of metal.

Mr. Airy to Mr. Bell.

Royal Observatory, Greenwich,
December 24th, 1852.

Dear Sir,

Since the meeting of the Southern Telescope Committee, I have repeatedly considered the subject of equatorial mounting of the telescope. And I think the probability of this being made practicable and efficient is so great, that I trust that the Committee may be induced to suspend any positive decision until they shall have discussed special plans.

I hope to be able to prepare a model in time for exhibition to the Committee; meantime I may explain that the principles which I propose to adopt are the following (of which the first and second have been long entertained by me as necessary for the safe and convenient use of a large reflector).

1. The mounting, as regards the support of weight of the telescope and the keeping of the same edge of the speculum always at the bottom, is to be strictly that of an altitude and azimuth instrument.

2. An equatorial construction is to be used, not for supporting any part of the weight, but for guiding the telescope.

3. This is to be so connected with clock-work that the telescope will move truly in hour-angle, without requiring any adjustment of the clock-work for different polar distances.

4. The eye-piece and micrometer-work are to receive a motion exactly corresponding to the rotation of the meridian as seen in an altitude and azimuth instrument; so that the micrometer-wire, once placed to make a certain angle to the meridian, will remain making the same angle with the meridian.

Believe me, dear Sir,
Faithfully yours,
G. B. Airy.

Thomas Bell, Esq.,
Secretary of the Southern Telescope Committee.

Dr. Robinson to the Earl of Rosse.

Observatory, December 28th, 1852.

My dear Lord,

Colonel Sabine has sent me Mr. Nasmyth's letter on the subject of the southern reflector, and your remarks on it.

If I comprehend Mr. N.'s drawing rightly, his polar axis consists of two parallel frames, united at top and bottom by two transverse pieces, between which the telescope is suspended. There seems no provision for seeing any part of the sky near the pole; and I think the axis would be peculiarly liable to torsion. The idea of turning the whole telescope round *its* axis, involves, I think, some difficulty. It must be provided with two systems of rollers, which under the open air may not always act well; and it may perhaps be easier to make the small speculum and eye-piece revolve.

I quite agree with Mr. N. in wishing not to shorten the focus beyond your proportion of 9 diameters. I have succeeded very well (15 inches) in 7·2, but with far greater difficulty than with 9. Indeed, even when the process failed so far that the whole aperture was not good, the proportional part of it was mostly excellent. I hope therefore that the 4-foot may not be attempted with less focus than 36 feet.

I enclose you a tracing (which return when you have looked at it) of two schemes which were discussed by Grubb and myself in reference to the southern reflector some years since. In that marked R, I proposed to make *the speculum box itself* the declination axis, providing it with journals and a circle, and to have a short and massive polar axis. The tube above the box was to be merely an open skeleton, framed so as to be stiff enough to support the ocular part and small mirror. This would bring the centre of gravity very near the speculum. Grubb did not like this, and suggested another marked G. The polar axis here has its upper bearing about 6 feet diameter, but is cut at top so as to let the tube pass in for reaching the pole. This would a little weaken the axis there, but from its great diameter the strength would be abundant. It would also interfere with the motion of the instrument at more than five hours from the meridian.

I think none can gainsay your remarks as to the advantage of an equatorial movement. Work such as we contemplate is facilitated almost beyond estimation, when the object to be drawn is kept immoveable in a given part of the field; and the fatigue of signalling or shouting to the assistants when the movement is by hand, absorbs no small portion of the observer's powers.

And I think your preference of the "German" form over the "English" admits of as little dispute; that one of the latter form *may* be steady if an enormous mass be given to its moving parts, is obvious, but it must be remembered that such mass increases most materially the difficulty of using it, and that the increased friction must cause great additional strain on the moving parts. It is probable that the metal of the Liverpool mounting, if applied in a way

similar to Mr. Cooper's instrument, would carry a three-feet reflector. I have been told it weighs six tons. Now the weight of Mr. Cooper's is—

	Cwt.
The equatorial part	25·95
Tube, object-glass, eye-tube ...	9·12
Counterpoise.....	12·42

Of the first the polar axis is about 18 cwt., or twice the weight of the telescope. Now I have been familiar with this instrument's working during many years; it is very easily moved, and perfectly steady. The shelter of the lower half of its tube by the surrounding wall, in my opinion, rather increases the power of the wind on the upper part to shake, as its pressure is unbalanced. In the case of a reflector, the influence of the wind would be much lessened by making its tube a skeleton, and I think that *instead of the counterpoise*, there might be put at the other end of the axis *an apparatus for supporting the observer*. *He* should be counterpoised so that the frame could be lowered to get him into his chair, then raised and clamped in his place. Mr. Cooper's equatorial cost, I think, about £400.

The Germans themselves are quite satisfied with Reichenbach's construction, and that of Munich is, I know, used in the open air. Their polar axes are solid and made of steel, but it is certainly better to throw the material into a tube. Short bearings seem to be the secret; it is scarcely possible to make a stronger axis than that of your equatorial, but the length makes it twist. Need a polar axis be longer than the mandrel of a lathe? I forgot to mention that Grubb is getting ready two equatorials for the Dublin Exhibition; one a model of that noticed above, with a 15-inch reflector, the other an improvement of Mr. Cooper's, with an 8-inch achromatic of his own grinding. I will report to you, or perhaps try them with you.

T. R. Robinson.

The Earl of Rosse.

P.S. Of course anything from Airy must be considered; but I don't expect much improvement on the equatorial. Before I had my equatorial mounting for the reflector, I found that with the old 9-inch Herschel which I found here, and its rickety stand, I could take no measures with the micrometer, and in 1830 I made an alteration, which may possibly be what Airy is devising. The telescope was set on a circle revolving on balls by a joint something like your 6-feet one, and was counterpoised. But the horizontal slide of Herschel's stand was connected with a circle, by means of which it could be inclined to the angle of position, and clamped there. This angle was got by a table of double-entry, of which the arguments were the azimuth and altitude, both given by the instrument. The slide-screw gave the movement; it acted very fairly as a substitute for the length of the rack, about 30 minutes at the equator; but, as you may suppose, I was not sorry to get a true equatorial. I really do

not see any insuperable difficulty about the mounting. A common turn-table with its edge on rollers would bear forty tons, and there would be no great trouble in setting strong standards on it to carry the tube. Remember, that one does not want the delicacy of a meridian circle here, only motion.

Mr. Nasmyth to the Earl of Rosse.

Bridgewater Foundry, Patricroft, near Manchester,
December 30th, 1852.

My Lord,

Having made out a more careful drawing* of what appears to me would be the most suitable construction for the proposed 4-foot reflector, I take the liberty to send it you, as it may so far tend to establish the best arrangement for such an instrument.

I am very fain to have it mounted equatorially, as there are such vast advantages in that construction for the steady observation of any selected object of which it may be desirable to make careful drawings, and as when once the instrument was set upon the object in question, the clock-work apparatus would do the rest. The clock would require to be of very considerable power, but I see no difficulty on that head. By having suitable means whereby the assistant could cause the tube to revolve in the square frame cage, the eye-piece can be kept horizontal at all times, which would also be equally advantageous for the easy set of the metal on its under edge-bed. The jointed cell for the metal is a very handy arrangement, whether for opening the clock for sliding in the disc of zinc as the protector, or for the easy removal of the cell and metal by truck and railway to the polisher when it is required. A truck with an elevating top would do this very handily. The three elevating screws would be found very handy in getting the cell into connection with the joint on the tube; the truck might be so arranged as to run direct into the polishing machine, and so avoid all the risk and trouble of transferring the cell from it to the machine. The tube would require a chain to retain its position when the counterpoise action of the cell and metal was removed. Pray excuse this very hasty letter, and believe me I am

Yours most respectfully,

James Nasmyth.

To the Right Honourable
The Earl of Rosse.

Mr. Lassell to Mr. Bell.

Valletta, December 30th, 1852.

Dear Sir,

I was preparing some reply to the circular of the Southern Telescope Committee, dated the 1st instant, which has been forwarded to me here, when I received your second circular of the 15th instant.

* May be seen at the Royal Society's Apartments.

It appears to me that the question now wears quite a new aspect, and it will be for the Committee to determine whether they will carry out their original purpose of erecting an eminently large telescope in the southern hemisphere with every appliance which their united wisdom can secure—or whether they will, as a preliminary step, elect an observer, and commit to him the conduct, arrangement and carrying out of the whole affair.

Concluding that they will adopt the former resolution rather than consign so great an undertaking to any single individual, however able and accomplished, I yet see all but insuperable difficulties in adequately realizing so grand a scheme.

If there were any manufactory in the world which could furnish completely mounted a 30-inch object-glass of similar quality to the 15-inch Munich glasses, I should say the best thing would be to give the order at once. But as that is not the case, the proposed reflector of 4-feet diameter is the best that can be chosen (in my judgement), though I would rather not shorten the focus beyond 35 feet; conceiving it to be difficult to get the figure, otherwise, good quite up to the edge; and it seems to me unwise to incur all the difficulties of a colossal telescope, and then be obliged to cut down its dimensions.

I know something of the efficiency of a 2-foot reflector, when brought up (I believe) extremely near to perfection in its figure, and furnished with a convenient and steady equatorial mounting driven by clock-work. It would not be worth while for the Committee to erect a telescope which should not be at least in comparative efficiency to this, *as two to one*; and in order to render the 4-foot equal to this it must have the same conveniences of mounting; for any deficiency in this respect will really have the effect of a reduction of the size and power of the telescope. I have seen a sketch of a plan of equatorial mounting by my friend Mr. Nasmyth, which is simple and well-calculated for such a latitude as the Cape; but I think it would never do to suspend the observer from the eye-piece; for the slightest motion of his body, such as the extension of his arms to adjust the focus, alter the polar distance, or turn the micrometer-head—even his very pulsation, would be so communicated to the telescope as to be visible in the image with the high powers proportioned to such a telescope. Besides, a very moderate breeze would equally prevent delicate observations, and therefore the telescope must have a revolving roof for protection from the wind, and affording a status for the observer. By placing the instrument a little out of the centre, 50 feet might do for the diameter of such a dome, and the few degrees of sky near the zenith thus sacrificed, might be recovered by a shutter, only to be used on requirement, which would extend the opening over the centre of the telescope when vertical. From the experience I have had in the construction of domes 15 and 30 feet in diameter, I am satisfied that the same principle of construction might be extended to 50 feet, and that no very formidable difficulty would present itself here.

The mounting and application of driving motion would be readily

executed by estimate according to plan, by any of our more intelligent engineers without any serious obstacle.

But the casting, grinding, polishing and supporting of the great speculum, constitute the grand problem the solution of which it is not easy to see. For the successful accomplishment of the most essential part of the work, there must be *perfect unity of design and execution*, and therefore it must, in my opinion, be executed entirely under the direction of a single individual, who will nevertheless be able to avail himself of any suggestions of others which he may deem judicious. I will mention some of the requirements which I think demand this condition. Two specula must be cast, 4 feet in diameter, and not much less than 6 inches thick, each weighing therefore about 3500 lbs.; and supposing the difficulties of casting and grinding got over without accident, there comes the greater difficulty of polishing and bringing up to the requisite accuracy of figure, and most especially, *of defining power*. For this I would submit no one is competent who has not had considerable experience in the art itself; the cleverest engineer would be here at fault; and indeed it is a process which has so rarely been attempted on a large scale, that I think it would not be easy for those best acquainted with the subject to say when the greatest practicable amount of perfection was obtained. It is obvious that some machine must be used, and that, in all probability, repeated polishings and trials must take place before the required accuracy of figure and defining power can be combined in their highest degree. Both specula therefore must be figured and polished in the first instance, examined, and the worst of them taken again to the polisher. For this examination with sufficient advantage, the entire mounting of the telescope must have been previously erected; in fact the whole telescope, dome and clock-work and all, must be erected completely on the place of operation, just as it would have to be in the place of its ultimate destiny, before a satisfactory final result can be secured.

Another formidable difficulty I have yet to mention—the sure support of the speculum to prevent its bending, and secure its uniform action in all positions. In addition to the system of levers on which the speculum is to be ground and polished, and which secures this condition when horizontal, another system, similar or equally efficient with that I have described in the British Association Memoirs for 1850, must be applied, to come into action as the telescope is depressed from the zenith, and prevent the speculum *cringing down* as it is turned on edge. Without this latter apparatus the image of a bright star gives indisputable evidence of the speculum taking an oval form, from the impossibility of bringing the rays from the ends of two diameters of the surface of the speculum at right angles to each other—to focus at once. Hence the intolerable annoyance which I am accustomed to call "crossing," visible enough (if the metal otherwise define well) on Jupiter or Saturn as well as on fixed stars. To apply this apparatus conveniently, the specula should have either a number of blocks, or indentures, cast on the

back; the latter would be best, in order not to interfere with the smooth grinding of the back, which would be desirable.

Supposing then the telescope at last erected complete, contiguous to the site of the workshop and pronounced perfect, would there not be some compunction at taking it down before some experiment of its magnificent powers on the heavenly bodies should be made? However, letting that pass, and looking to its ultimate object and destination, I don't see who is to manipulate with such a telescope but the maker. I believe that hitherto no very large reflector has done any good service but in the hands of the artist who constructed it. At least if there were an astronomer, *par excellence*, to conduct the observations, the maker must accompany him, and, I should say, take out the polishing machine and apparatus for renewing the surface in case of accident or deterioration of the lustre by exposure.

William Lassell.

To Thomas Bell, Esq.,
Secretary of the Royal Society, &c.

P.S.—The suggestion of a great altitude for the site is good. Perhaps *Quito* would do, but how is the telescope to be got there?

Sir J. W. Lubbock, Bart. to Mr. Bell.

March 12th, 1853.

My dear Sir,

As I am requested so to do I offer the following remarks, with very great diffidence, not having had any experience either in making or using large instruments. Besides, almost every point is so ably handled in various letters from members of the Committee that the subject is pretty well exhausted. There are, however, one or two which have not been noticed.

1. Surely the Government will never authorize the establishment of a great national observatory at Quito, or any point not in our own territory. If, then, Australia is also out of the question by reason of the abnormal state of that colony, we have only the Cape to consider, and so that point would be disposed of.

2. Mr. Smyth attaches great importance to elevation; this I can quite appreciate; but the elevation which he contemplates will, I think, never be reached without disadvantages which will outweigh the benefit; and there are two other conditions I should consider as essential,—

1. The non-contiguity of the sea or of much standing water.
2. The non-contiguity of any manufactory or large town.

I confess the impression made upon me after attentively reading all the letters, and especially Mr. Lassell's, is that it will be impossible to carry out the plan with a reflecting telescope (I mean of 4-feet diameter); secondly, that it must be equatorially mounted and sheltered by a dome; thirdly, that *if* it can be done on the mag-

nificent scale apparently contemplated by the Committee, we had better begin by having it in the northern hemisphere first.

I apprehend (but perhaps I may here be in error) there is plenty of work at the Cape for an instrument of such comparatively inferior power as the Cambridge (America) telescope, and up to this point the erection of an observatory would present no insurmountable difficulties.

In Mr. Nasmyth's mounting, in addition to the other difficulties noticed in the letters, the chair of the observer would act at such a mechanical advantage, that I suppose it might affect the tube, but perhaps it is thought that a slight strain on the tube would not be injurious to the observation.

I am, dear Sir,

Yours faithfully,

Thomas Bell, Esq. J. W. Lubbock.

Sir John Herschel, Bart. to Mr. Bell.

My dear Sir,

I am very sorry to have kept the papers relating to the telescope proposed to be established in the southern hemisphere for the observation of nebulæ much longer than I could have contemplated; but in truth I have been so much engaged of late that I have found all consecutive thought on the subject impossible, and can now only give what has occurred to me on their perusal in a very unconnected form.

I must, however, preface my observations with a distinct statement that I am by no means sanguine as to the success of the project—I mean as to the attainment of results at all commensurate in scientific importance with the imposing character of the undertaking as a national one, and the *very large* outlay of the public money which it will call for. It is by no means therefore as an advocate for the measure (on which, in an earlier stage of its progress, I have expressed my opinion more at large) that I have assented to my name being placed on this Committee, but that I may not be considered as disposed to withhold (should the project be persevered in) such slender information and practical suggestions as my experience in observations with large reflectors, directed to this object, may enable me to offer as to the kind of instrument and the mode of observation best suited to the purpose. It seems above all things necessary that, before proceeding a step in the affair, its movers should have before them a very clear perception of the sort of instrument required, and its adaptation to the work in hand.

There are only two constructions specially indicated in the papers which have come into my hands—those of Mr. Nasmyth and of Mr. Smyth. To the former of these it appears to me that there exist serious objections, as follows :—

1. The polar axis (the mounting being equatorial), as exhibited in the figures, however well trussed, cannot but have some degree of

spring perpendicular to its length, which will ensure tremor whenever there is any wind or any movement of the observer in his box. Although the telescope is supported on its centre of gravity, yet owing to the different lengths of its two portions on either side of the point of support, and the very different disposal of the weights, the propagation of such tremors through them cannot be synchronous, and must therefore result in momentary flexures to and fro of the tube, giving rise to oscillations in the line of collimation, and consequent tremulous apparent movements of the image.

2. Such a frame as that of the polar axis, divided down its whole length, and that length a considerable one, cannot but be liable to torsion, since, however well the sides may be trussed, they will ultimately act as two flat ribands. The effect of such torsion is conveyed *entire* upon the line of collimation; and as the upper and longer portion of the tube will be much more exposed to wind than the lower and shorter, I should expect the tremors so produced to be exceedingly obnoxious. The hold of the wind on such an apparatus, totally unsupported except on the centre of gravity, will be most formidable.

3. As the instrument deflects out of the meridian, the observer will have to look to the security of his position, and to work a mechanism to keep his box vertical. It does not appear by what mechanism this is to be performed. The mere weight of the observer and box will not do it, and if they could they must overdo it, and fall beneath the tube. I can imagine none free from objection, and the line of collimation will be kept in a constant state of unsteadiness and derangement from this cause, which may carry it out of the reach of small corrections.

4. The observer will have to balance himself in his box before elevation into the air by adding or subtracting counterpoises. He is insulated aloft, at an elevation from which he cannot descend without the aid of an assistant, at whose mercy he is, and who may be negligent or sleepy. His position would certainly not be an enviable one.

5. The speculum and its supporting frame hang on a chain by an oblique pull on the edge, being sustained also by a hinge below. I can imagine nothing more objectionable. An enormous power, many times the weight of the mirror, would be required to be applied to this chain to keep the mirror-frame tight home as screw-work would do.

I am hardly engineer enough to criticise Mr. Smyth's gigantesque construction of a 100-feet equatorially mounted reflector of riveted iron plates, which might perhaps have that sort of stability which any vast aggregation of materials may possess under moderate winds; though, from the extent of surface exposed to such gusts as prevail at the Cape, I should not be very confident even in this respect. But there are several points which may be mentioned as objectionable about it.

1st. There is no provision in it for keeping one side of the mirror always uppermost, and the observer in a vertical position. Such an

adjustment is essential, and I believe can only be adequately secured by the altitude and azimuth movement.

2nd. Mr. Smyth considers it sufficient to destroy the evils of heated air that the chamber at the mouth of the tube should be duly ventilated to keep the personal heat of the observer from passing across it; and, as regards the rest of the tube, he does his best, by a cellular structure of its *parietes*, and by enclosing it in an outer triangular case, to give a durability and uniformity to *its* temperature, which in my opinion must infallibly secure a continuance of that slow spiral movement of the internal air, by the interchange of air within and without, through the upper aperture which is the greatest enemy to distinct vision in reflectors.

3rd. The observer's access to his aërial chamber is by climbing up a sort of chimney, with ladder-steps in one angle of the triangular envelope. In his figure this chimney is $\frac{7}{18}$ of the internal diameter of the tube. For a 6-feet reflector this would afford a sufficiently roomy, though very inconvenient passage; but for a 4-feet size would allow barely room for a man to creep up, unincumbered with any books, drawing-apparatus, &c.

In the construction of a large reflector I would press the following points as essential to be attended to:—

1st. A very perfect internal collimating telescope, with means under the observer's hand of adjusting fiducially the line of collimation without the intervention of an assistant. In my "Cape Observations," the means of doing this most effectually, most simply, and to any required degree of precision, are pointed out,—subject of course to improvement in point of workmanship and mechanical arrangement.

2nd. A perfect support of *every point* of the mirror on an elastic bed. I am by no means entirely satisfied (speaking with all due respect) with Lord Rosse's ingenious contrivance of triplets. It is mechanically complex, weighty, and, after all, only supports a definite number of points. I should prefer a bed composed of a great many layers of horsehair-cloth, or frieze or felt, of such united thickness and softness that their total compression by the weight of the mirror should exceed by a great many times the minute amount of flexure undergone in the solid back-support on which they and the mirror both ultimately rest, *thereby annihilating the influence of such flexure on their supporting power.*

3rd. The mirror should be so sustained as to rest *against* and *upon* this bed, under any circumstances of inclination to the horizon, with perfect freedom to rise and fall, advance or retreat, without change of its parallelism, which may be accomplished by suspending it in the mode described in my "Cape Observations" (Introd. Art. xv.), in a jointed ring from a point above its vertex. In this, it must not rest on a single point at its lower edge, but should be supported over a large arc of from 60° to 90° of its inferior circumference on soft and compressible material, interposed between it and the ring. *Such a mode of support however necessitates the conservation of the horizontality of a fixed diameter of the mirror.*

4th. I should greatly prefer a skeleton tube, or one (if of iron plate) so pierced with large apertures as to allow of a completely free communication between the interior and exterior air throughout the whole length of the column, except perhaps in the very immediate neighbourhood of the mirror, for a foot or two from its surface, where it may not be needed. Long experience has satisfied me that the establishment of ascending and descending currents of hot and cold air in a long inclined tube, open at the top, chimney-fashion, is one principal cause of indistinctness of vision and deformity of image.

5th. The destruction of tremor is better secured by several cords of unequal tension, or by several steadying rods of unequal strength, weight and elasticity, than by one single one. It is to this principle that the exceeding absence of tremor in the 20-feet reflector used by me in my observations of nebulæ is attributable. The suspension of the upper end of the tube by a tackle of pullies, which, by their friction and the stiffness of the cord passing over them, never permits all its reduplications to have the same tension, causes them to vibrate out of unison with each other, and so to destroy each other's effect in communicating or perpetuating tremor.

A form of mounting has been indicated by the Astronomer Royal, in which an altitude and azimuth motion of the tube otherwise free, and permitting a range over the whole sphere, is limited to a given parallel of declination, by determining the distance of the lower edge of the orifice of the tube from a fixed point (which may be called the polar point) in a line parallel to the earth's axis, and passing through the common centre of the horizontal and vertical movements in the lower support of the lower edge, or in the centre of gravity of the instrument (if so sustained). If the difficulty of communicating a clock-movement to a tube so guided could be completely overcome, there would remain that of keeping the micrometer-wires in the field of view in a position invariable with respect to the parallel of declination. Mr. Airy speaks of a mechanism to this effect, but I have not had an opportunity of inspecting his model.

With an altitude and azimuth mounting, and with an object on the meridian, there could be no difficulty in communicating to the tube a clock-movement in the direction, and with the velocity corresponding to the parallel of declination, at least for a few minutes. Were this done, a photographic impression could be obtained, with this, as with an equatorial mounting.

I should rely on photographic processes to impress on paper a skeleton picture, or the images of *the stars only* which might accompany or be disseminated over a given nebula, or those of a cluster to be delineated, and trust to the eye to fill in this skeleton, for which purpose perfect fixity of the object in the field of view would be of no importance. I have no expectation that such processes could be applied to the actual representation of the nebulous parts of such objects, so as to give the true forms of nebulæ as they appear to the eye in the telescope. The visible contour of a nebula

varies from telescope to telescope with the illuminating power of the instrument, while that of a photographic impression (supposing one possible) would depend essentially also on the time of exposure, which acts in this case as increase of light would do, though, as it does not follow by any means (and the contrary is even true for bright lights) that (time of exposure) × (intensity of illumination) is proportional to (intensity of impression), there would be no security that the gradation of light in the several parts of the impressed image would be preserved as in nature.

On the other hand, I have no doubt that satisfactory photographic images of clusters of stars or the stellar assemblages in nebulæ may be obtained and multiplied, and become most valuable aids in the depiction of the latter class of objects. The telescope ought therefore to be provided with fit and convenient attachments for the necessary photographic apparatus, and the observer should be an expert photographer.

Whatever may be done in the way of delineating the forms of known nebulæ, the discovery of new ones by zone observations, whether conducted by meridional sweeps, or by the application of the equatorial motion, as well as the revision and perfecting of the places assigned to those already catalogued; in short, the formation of a complete catalogue of nebulæ, ought to form a leading feature of the observations to be made with such an instrument, and a large portion of the stock-work of the establishment.

As the construction and working of the telescope must be experimental in great measure, I should regard a rehearsal of the observations in England as a quite indispensable preliminary to their ultimate prosecution in the Southern hemisphere. It is quite of equal importance to place on official record exact delineations and catalogued places of northern as of southern nebulæ. The accuracy of such delineations could be tested on the spot, and officially reported on by a Committee appointed for the purpose, and confidence thus secured to the results subsequently obtained out of the reach of such a check. In a word, the northern hemisphere should be first dealt with, and that completely and effectually; and in the process, and within reach of amendment, the whole course of proceedings might be perfected, and the necessary experience acquired. Five years devoted to this (of which the two first would be chiefly occupied in *roughing out* the work, and getting the apparatus in trim, and the three last in a complete *resumé*) in the clear climate of the south-east of England would be time excellently well bestowed, and would shorten the period to be spent in the south.

I remain, my dear Sir,

Yours very truly,

Thomas Bell, Esq. J. F. W. Herschel.

Dr. Robinson to Mr. Bell.

Observatory, Armagh, April 10th, 1853.

Sir,

I have received from Mr. Weld a portfolio containing the Correspondence of the Telescope Committee, with a request that I would transmit to you any remarks which occur to me from its perusal. These relate to the construction of the telescope, its mounting, and its site. Before entering on them, however, I must express my belief that the opinion expressed by several members of the Committee as to the difficulty of making a 4-feet reflector is exaggerated. I have seen so much of Lord Rosse's operations, that I feel authorized to say this, *absolutely* for a 3-feet reflector; with high probability for one of four. And were the difficulty and the uncertainty even as great as those gentlemen suppose, *that is the strongest reason for pursuing this our purpose.* It is only by the reflecting telescope that we shall reach the remotest parts of the visible universe. There is no likelihood that an achromatic of 3 feet (the equivalent of a 4-feet reflector) will be made in the next century; if made, it must be of enormously greater cost, and will be embarrassed by the evils of flexure and the polarizing action caused by pressure. It is therefore specially desirable that the reflector shall be made as perfect as possible; and never was a better opportunity offered than now, when we shall be authorized to experiment under the guidance of Lord Rosse, Sir J. Herschel, Mr. Lassell and Mr. Nasmyth.

1. *As to the telescope*: I wish to suggest whether it may not be desirable to use Cassegrain's form instead of Newton's. No light is lost, for M'Cullagh and others have shown that the reflexion from metals is less intense at 45° than nearer the perpendicular. I have much experience of its power with 15 inches aperture; and can mention as an important practical fact, that slight errors in the large speculum can be corrected by the small one, which is ground and polished on a miniature of Lord Rosse's machine. This construction has the great advantage of not requiring any apparatus of a complicated character to support the observer, and of shortening the tube nearly one-fifth.

In reference to the mode of supporting the speculum, three methods are proposed. Sir J. Herschel's consists in letting it rest on an elastic bed, uniformly bearing it; Lord Rosse bears it by friction balls on a system of equilibrating levers behind, and a ring for the edge also connected with counterpoises; and Mr. Lassell applies a second system of levers behind the mirror, which sustain it at all inclinations. The first of these is liable to this objection, that the speculum cannot be polished on its bed, as it must be immersed in water; either of the others may be used; but I would direct attention to a remark of Lord Rosse's, that the speculum may perhaps be cast with ribs at the back so as greatly to diminish its weight. Were these arranged so as to intersect at the points of bearing, I am disposed to think half the metal might be saved.

Sir J. Herschel speaks strongly in favour of a skeleton or open-

work tube; which will both diminish its weight and remove the most injurious of the influences which interfere with the definition of reflectors, the circulation of irregularly heated air in the tube. In fact, owing to it they never act perfectly except when they are at the same temperature as the external air. If the tube be suspended at its centre of gravity, its upper portion has only to bear the small mirror, and in the Newtonian the eye-piece also, so that it needs no great strength; and its diminished surface will give but little hold to the wind.

I see no reason why any one diameter of the speculum should be kept always vertical, but rather the reverse. Whenever the telescope is vertical, all pressure is removed from the edge; now on depressing it, if the strain is restored to the same point of the edge, it is much more likely to produce a permanent change of figure than if its action is distributed over a wide range.

2. *As to the mounting*: I am decidedly for an equatorial, and see no insuperable difficulty in it, if, according to Lord Rosse's recommendation, we adopt the German construction, and possibly also that mixture of cast and malleable iron (a sort of steel) which is stated in the Report of the Commission on Railway Structures, to be $\frac{1}{4}$ stronger than cast iron. Sir J. Herschel's objections to the constructions proposed by Mr. Nasmyth and Mr. C. P. Smyth appear quite conclusive. That of Mr. Airy, of which I have recently seen a notice in the Astronomical Society's Notices, seems too complicated to be quite satisfactory; and I fear the bar apparatus at the mouth of the telescope would injure definition. I may mention in proof of this, that having supported the small mirror of the Armagh Cassegrain by three very thin radial bars at 120° asunder, I was obliged to remove them, as the stars were shown with three minute rays bisecting the angles.

Mr. Lassell's opinion is certainly of great weight, yet I continue to think that large telescopes should be worked in the open air; I have tried a 15-inch reflector and a 12-inch achromatic thus, and also under domes, and very much prefer their action in the first case. Nor need there be much apprehension from wind. Mr. Cooper's equatorial gives a fair test of this; the instrument has its tube half-screened by a wall, above which 13 feet rise, exposed to the unbalanced pressure of the blast; but it is very steady even in high wind. Whatever construction be adopted, should (if new) be tried first on a tolerable scale with a working telescope.

3. *Site.*—It must certainly be on our own territories, unless we reject all past experience. As to latitude, I should not like it near the equator: we have no information as to the practical working of equatorials there, and I think the circumstances which in Europe make that instrument more liable to unsteadiness than those of altitude and azimuth, would tell with more power when the polar axis is nearly horizontal. There also, the poles and polar regions of the sky can scarcely be observed. On this account Simla and even Ceylon are objectionable. The Cape is unexceptionable in this respect; but both from what is stated by Sir J. Herschel in the preface to his

observations, and from the letter of Mr. C. P. Smyth, I doubt its fitness as an observing station for a reflector. In particular the fine dust on which the latter lays so much stress (and which is so widely diffused in Africa that even at 1000 miles from its western shore it falls on ships) seems most inauspicious.

I wish we had some precise data to estimate the effect of height; the researches of Forbes, with which alone I am acquainted, refer to solar *heat* and not to *light*, and do not enable us to separate the absorbing action of vapour and air. I am inclined to believe that the first is much more opake than the other; and if so, elevations of five or six thousand feet are quite unnecessary, as the great mass of vapour lies below. From observations which I made at Munich (not 1800 feet above the sea), I think that 2000 will be fully sufficient; for I consider it absolutely necessary that an establishment such as we contemplate be easily accessible, and within reach of the appliances of civilized life; the very expense of its maintenance will be greatly increased if it is fixed on some remote table-land or insulated mountain summit; and the rapid communication with astronomers at home becomes scarcely possible. In respect to height, we must also keep in mind that the region of condensation is to be avoided. On the whole, I am compelled to think this element of very second-rate importance, or at least that it ought not to be predominant in deciding the question. If we wait till we know the ratio of a telescope's performances at the sea-side, and at 10,000 feet in the same climate, and then till we find a convenient mountain, we shall leave the performance of an important duty to another generation. If we can find a climate as good as that of Nice even at the sea-side, let us take it; and I am confident a 4-feet then will do more on nebulæ, than any existing achromatic on the top of the Himalaya (if an observer could live and work there). I would, however, suggest the Mauritius as possibly offering a favourable position. I believe the central plateau is about 2000 feet high; the climate is described as fine, and from its position to the *east* of Africa it will be free from dust.

Before concluding, I would direct the attention of the Committee to a remarkable passage in Sir David Brewster's letter, from which it appears that the application which, as President of the British Association in 1849, I made to Lord John Russell on this subject, never reached him, but was summarily disposed of by some irresponsible official. The lesson I hope will not be forgotten by any who, as the representatives of our great Scientific Societies, may have occasion to communicate with Government.

I have the honour to be,
Your obedient Servant,

T. Bell, Esq. T. R. Robinson.

Observatory, April 29, 1853.

P.S.—Regarding the size of the small mirror in the Cassegrain form:—the lowest power that will use the whole pencil of a 4-feet is 220; assuming this, and that the aperture of the small mirror and

the hole in the large, and the diameter of the field lens of the Huyghenian eye-piece are the same diameter, I find, assuming Lord Rosse's proportion of aperture to focal length $\frac{1}{9}$,

Field	15′ 38″
Distance of mirrors	29·14 feet.
Focal length of small mirror . .	8·75 feet.
Aperture of it and diameter of hole .	0·76 feet=9·15 inches.

The diameter of the small mirror in the Newtonian form is only 4 inches; but the difference is insignificant in 4 feet; and besides the central part of a speculum is in practice not the best.

Grubb is grinding a 12-inch achromatic for the Dublin Exhibition; but I scarcely think he will be ready with it. The equatorial for it, however, is ready, and I think no doubt will exist as to its efficiency when it is seen. He grinds the lenses by a new and very simple machine, which acts without forming rings on the glass (Andrew Ross's great difficulty), and *if* it does as well for specula, will be an advance. I shall be curious to see how this object-glass acts; if it succeeds, there is some probability that he will try even larger sizes. But the *two glass discs* alone for a 24-inch would cost £1000.

E. J. Cooper, Esq. to Mr. Bell.

Markree Castle, April 29th, 1853.

Sir,

I beg to acknowledge the receipt from Mr. Weld of Dr. Robinson's remarks on the subject of the proposed Southern Telescope, written after his perusal of the correspondence contained in the portfolio forwarded to me, and returned by me to the Royal Society.

I have felt very great diffidence in offering any observations on this matter, particularly as my acquaintance with reflecting telescopes is limited; and I have hitherto confined the humble expression of my opinion to that branch of the inquiry relating to the site of the observatory.

However, as it appears to be the wish of the Committee that each member of it should be consulted, I do not think that it would be respectful to the Royal Society and British Association were I to withhold any remark that might occur to me. So far as I can understand the wishes of the Committee, they desire,—

(1.) A telescope not inferior to a 4-feet reflector, to be used principally for the examination and drawing of nebulæ.

(2.) The selection of a site on which such a telescope should be placed.

The first of these queries involves the considerations,—

a. Whether the telescope should be a reflector or a refractor.

b. The mode in which the telescope should be mounted.

c. The expense not only attending its first cost, but the permanency of its value, and also the maintenance of it in working order, together with the establishment connected with it.

(1.) As regards the selection of the telescope, it appears to me that the determination to have one of not less power than a 4-feet reflector evinced that the choice of the Committee had fallen upon a reflector in consequence of the difficulty, if not impossibility, of obtaining a refractor of equal light; and this apparent prejudgement caused my original hesitation as to taking any part in the deliberations of the Committee. I am free to confess that I see no prospect whatever of acquiring an object-glass affording light equal to a 4-feet reflector, even in its mean condition; but perhaps I may be excused for making some comparisons between the performances of the two instruments. Previously to Sir J. Herschel's voyage to the Cape, I sent him drawings of the great nebula in Orion, and of 51 Messier; and in his reply to my letter he stated his belief that his 18-inch reflector had more light than my 13·7-inch refractor; but that my refractor must be remarkable for its concentration of light. This opinion of Sir J. Herschel arose from my sketches of the nebulæ having been made under a high power, which, while bringing into view minute stars, invisible with a lower power, obliterated at the same time portions of the nebulæ which were seen with a lower power. Having compared my drawings under a low power with one he kindly sent me, I ascertained that with reference to light the instruments were nearly on a par. In his Cape observations Sir J. Herschel mentions his verification there of several minute stars seen by me, which his 18-inch reflector had not rendered visible to him in England. It would appear from these facts that a refractor giving equal light with a reflector, and in juxtaposition with it, would show stars better than the reflector. My belief has for a long time been that all nebulæ are clusters of stars, and in a great number of them stars are visible. It is of importance to note the position of these stars, for the purpose of detecting any dynamical change which may occur in the system of which they form a part. The exhibition of the spiral construction in some nebulæ presents a problem of intense interest to the physicist. It remains to be seen whether the practical astronomer will be able to furnish him with any materials towards its solution. The mere outline of a nebula will not be sufficient, as there might be an absolute permanency of form where the constituent star particles are in constant relative motion.

In point of definition I have always thought that reflectors had the advantage; but the drawings of the planet Saturn by Mr. Dawes and Mr. Lassell respectively (see *Ast. Nach.* No. 840),—the former by the aid of a $6\frac{1}{3}$-inch aperture achromatic, the latter with a reflector of 2-feet aperture,—go far to fix an impression on the mind, that achromatics, at least of moderate size, can be procured to exceed reflectors of much higher relative power. It is also worthy of consideration that Mr. Lassell himself thinks, that if a 30-inch object-glass of similar quality to the 15-inch Munich glasses could be furnished, completely mounted, by any manufactory in the world, the best way would be to give the order at once. I cannot help agreeing with Mr. Lassell in this opinion, and am not prepared at

present to say that such an object-glass could not be produced at Munich. But I shall be met here by an objection on the score of the expense, even supposing that other objections should be withdrawn. Having heard that manufacturers estimate the probable cost of object-glasses as the cubes of their diameters, the expense of a 30-inch object-glass would no doubt be formidable; but we have already an estimate by Lord Rosse for a 4-feet reflector, amounting to no less than £6000; and it must be remembered that in addition to this sum for the telescope and its mounting, there must be an annual charge against the Treasury of no slight amount for the salary of the polisher. I would also deferentially submit that the permanency of the value of an achromatic is greatly superior to that of a reflector. I think that it will be admitted that the German mounting of an achromatic of the size proposed by Mr. Lassell, would be far less expensive than that of a 4-feet reflector.

Having said thus much on the subject of refractors as compared with reflectors, I turn to the remarks made by the several members of the Committee on the proposed 4-feet reflector. It does seem to me worth while to consider Dr. Robinson's suggestion to adopt the Cassegrainian instead of the Newtonian form, particularly if the latter should be held to involve the suspension of the observer at the mercy of his assistant. I also incline to the opinion of Sir John Herschel and Dr. Robinson in favour of a skeleton tube, although what the latter gentleman states of my equatorial is perfectly correct. I am also strongly in favour of the telescope being used in the open air, not conceiving that any astronomer would think of observing, even under a dome, during the prevalence of a hurricane. The equatorial seems to me to be the only form to adopt with a view to satisfactory observations. I cannot help considering photographic representations of the nebulæ as secondary desiderata. I fear that only a rough outline could be thus obtained, and I doubt much that very minute stellar points in the nebulæ would be distinguishable by this process. If my conjecture be correct, these pictures would only serve as rough outlines for future work.

(2). *Site of the Observatory.*—I have already stated in my former letter that I was disposed to recommend high table-land in a latitude as near 45° as possible. It is reasonable to demand what has led me to this choice. I have myself made observations both on the sea-coast and inland, in various places in Europe between 67° latitude and the southernmost part, and in Africa as far south as the second cataract of the Nile, viz. 21° 52′. The most favourable places for observation I found to be at Nice, on the coast of the Mediterranean, and at Munich. Throughout the valley of the Nile the weight of the dews after night-fall was so great that even an achromatic telescope could not be used at an early hour. In mentioning high table-land, I had in my mind the position of Munich, the observatory at which place is 1660 feet over the sea. The sky here struck me to be peculiarly dark, much more so than at Nice, and this position has what might be considered a disadvantage, ranges of mountains within a short distance. I therefore had no idea of ad-

vocating a position higher than, say 2000 feet, and I agree entirely with other members of the Committee in their different objections to a much higher situation. There is one circumstance with regard to a very high situation that does not seem to have been taken into account, except impliedly by Dr. Robinson, viz. the effect of the rarefied air upon the observer himself. I have my misgivings as to placing an individual in so different an atmosphere from that to which he has been accustomed. The preservation of his bodily health, and consequently his visual capacity, is surely of quite as much importance as the perfection of the instrument he has to use. So far as I am informed upon the climate and geography of Tasmania, I certainly think that it is probable that it would best fulfil the conditions that seem to me to be most desirable as to site.

I have the honour to be, Sir,
Your obedient humble Servant,
EDWARD J. COOPER.

Thomas Bell, Esq.
&c. &c.

Mr. Airy to Mr. Bell.

Royal Observatory, Greenwich,
April 30th, 1853.

MY DEAR SIR,

I have received, and now return to Mr. Weld, the correspondence of the Southern Telescope Committee up to the present time. In sending you some remarks in continuation of the same correspondence, I cannot avoid commenting on some of the proposals of my colleagues; but I trust they will understand that I have no wish to criticise any further than is necessary for conveying my own opinions.

I. ON THE GENERAL COURSE OF CONDUCT OF THE COMMITTEE.

1. I think that great attention is due to the letter of Mr. Lassell, and especially to his discussion of the question whether the Committee should decide every particular, or whether the Committee should give their confidence to an observer who should decide every particular. I certainly incline, as I understand Mr. Lassell does, to the latter course. And this was my feeling when, at the Ipswich Meeting of the British Association, I expressed my opinion that no public step ought to be taken till we had decided on the observer. I conceive that all that we can do at present is, to collect information to assist the observer; and all that we can do hereafter will be, to hear his plans and remark on them, leaving the adoption or non-adoption to him; and finally, when a scheme is fully arranged, to propose it to the Government.

2. I share in some degree with Sir J. Herschel the want of very sanguine hopes of success, at least for some time. And I think with

Sir J. Herschel that some time ought to be given to trials in England.

3. Lord John Russell's non-recollection of the former proposal, to which Sir D. Brewster and Dr. Robinson attach great importance, was, I suppose, simply the forgetfulness of one of the ten thousand proposals which come before a Prime Minister. I have not now the British Association papers ; but I understood that the Memorial was addressed to Lord John Russell, and in that case (as it was not lost) it certainly went to him ; and I believe that a reply came from the Secretary of the Treasury, and in that case it was undoubtedly discussed at the Treasury Board.

II. On the Site.

4. The station which appears to satisfy best the desires of some Members of the Committee for table-land elevation with south latitude is the Mauritius. But its latitude is low (22°), and this, as I understood verbally from Lord Rosse, would be very injurious to the observation of South Polar Nebulæ.

5. As very great importance is attached by some Members to the comparatively untried circumstance of elevation, I would submit whether it would not be desirable that experiments should be made with a pretty large telescope specially for this purpose. If such a station as is afforded by a Scotch mountain would suffice, I would propose the summit of Ben Lawers as preferable to that of any other mountain that I know. The ascent to within 100 or 200 feet of the top is very easy, up a grassy slope without bog or crag, where a horse and cart could go all the way; and there is a comfortable sheltered hollow close to the top, in which I found the Ordnance party encamped the last time that I was in Scotland.

III. On the Telescope and its Mounting.

6. In the only detailed plans which have been prepared for the Committee (Mr. Nasmyth's and my own), it is proposed that the observer should be suspended to the end of the telescope, if Newtonian. Some Members consider this likely to cause unsteadiness. It is much to be wished that Lord Rosse would institute decisive experiments on this point.

7. It appears to me very desirable that, before we decide on any plan for operations out of England, we should determine by experiment in England whether photography can be applied, either to nebulæ or to stars. For the special purposes of this Committee, as well as for astronomy in general, I do hope that Lord Rosse may be induced to experiment on this,

8. I think that Dr. Robinson's proposal of the Cassegrain construction is well worthy of consideration. In reference however to the possibility of photography, I make the following remark (possibly unimportant). Supposing it likely that many photogenic rays might be lost at a second reflexion, I had arranged means in my model for receiving on the photogenic plate the image formed *immediately* by

the great mirror. If this be done with the Cassegrainian, the tube cannot be shortened as Dr. Robinson proposes. Moreover there will be loss of light, for the Cassegrain small mirror will probably be larger than the Newtonian small mirror, and cannot conveniently be made of silver as the Newtonian can. On the other hand, the Cassegrain gives great facility for the location of the observer, especially in my plan.

9. In my model, I exhibited a gallery or ladder for access to the observer's place, if suspended at the mouth of the tube; so that there is not really personal danger, or risk of being left to perish by a negligent assistant.

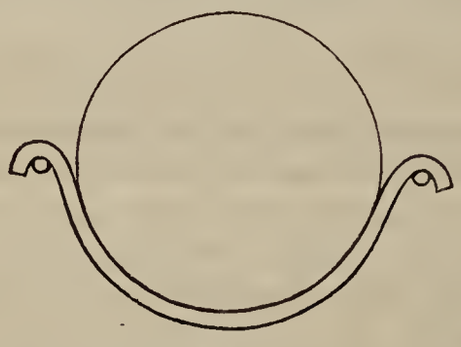
10. In deciding between different plans of mounting, I attach great importance to the holding the telescope by its *end*.

11. I should think a skeleton tube with spiral braces, very desirable.

12. The evidence of Dr. Robinson and Mr. Lassell seems to show that a 4-feet mirror cannot safely be made with focal length less than 35 feet. This will require serious consideration.

13. I think Dr. Robinson's remarks are conclusive on the advantage of Lord Rosse's mode of supporting the mirror; yet I am sorry not to take Sir J. Herschel's.

14. Dr. Robinson remarks that there is no need to keep the same edge of the mirror downwards, and that it may be even injurious. As regards the abstract condition of a mirror, I have no desire to question this; but as regards the inquiry before us, I think it is founded on a misconception. It is not abstractedly the *mirror* which Sir J. Herschel and myself wish to preserve with the same line always horizontal, but the *apparatus that supports it edgewise*. Suppose (as in Lord Rosse's and Mr. Lassell's telescopes) the edge of the mirror is carried by a hoop or chain, thus,



then if the telescope were twisted so that the mirror and its hoop-support should be in this state,

the definition of objects would be absolutely ruined.

15. Dr. Robinson has remarked on the injurious effects produced by bars in the mouth of a small reflecting telescope, apparently 9 inches aperture. Supposing that in a large telescope the arrangement of the bars is *similar* and their thickness *proportional*, then the measure of the diffraction phenomena *in angle* will be inversely proportional to the diameter of the mirror *in linear measure*; and thus a disturbance of image, which would be extremely injurious with 9 inches aperture, will be absolutely insensible with 48.

16. Professor C. P. Smyth's proposed mounting appears too large for us to entertain at present.

17. I contemplate a telescope in the open air as best.

IV. On the Exhibition of Results.

18. I do not deny the importance of Professor Smyth's proposal for engraving on the spot; but I am far more disposed to encourage the preparation of very careful drawings, and trust to fortune for engraving them, as far as might be necessary, in England.

I fear that I have not preserved very strict order in the arrangement of my remarks, but I hope that they may be intelligible.

I am, my dear Sir,

Faithfully yours,

Thomas Bell, Esq., G. B. Airy.

Secretary of the Royal Society.

Dr. Robinson to the Earl of Rosse.

Observatory, Armagh, May 6, 1853.

My dear Lord,

I have read the remarks of Mr. Airy on the Telescope correspondence, and concur entirely with many of them. Others however appear to me less convincing; and though I feel strongly how weighty his authority is, yet as my acquaintance with you has made me familiar with large reflectors, and a tolerably long life given me some experience of the ways of men, I submit to you my reasons for questioning in the cases where we differ.

1. Mr. Airy would appoint an observer, and let him decide every particular. Nothing is gained by this but delay. The Committee *must* fix the nature of the work to be performed; they are the proper persons to fix also the means of doing it. Whether achromatic or reflector be used, the present Committee possess more knowledge of the subject than can be found in *any one person*. Let us provide the telescope, choose the site, and an observer will not be wanting. *If Britain cannot furnish a qualified person*, let us carry out free trade and seek him at Berlin or Poulkova.

2. Mr. A. has not very sanguine hopes of success. I have, and for this reason:—your papers show how much a 3-feet adds to the knowledge of the Northern nebulæ, which had been obtained by an 18 in., even in (*pace dixerim* !) a bad observing climate. Assuredly

a 4-feet must do more for the Southern in the worst station than has been suggested.

3. My reference to Lord John Russell was meant merely as a caution to guard in any future application against irresponsible advisers of the Minister. The Royal Society's and British Association's recommendations should not be disposed of as they were in that instance.

4. Mr. A. thinks the Mauritius's latitude too low for South Polar Nebulæ. Our primary object is, I believe, the re-examination of Sir J. Herschel's Southern Nebulæ. Now in his catalogue there are but two whose altitude is under 30° at Mauritius, and one at that limit; and since your 3-feet is able to resolve part of the Orion nebula (at nearly the same altitude), *à fortiori* in the finer climate and at the greater elevation of the Mauritius, even it would be far more effective.

5. I like Mr. A.'s notion of Ben Lawers, and think it would be a proper object for the Royal Society to examine. The observations should be made at the base as well as at the summit, and the test should be the resolution of nebulæ. That of double stars is a different matter, being sometimes aided even by fog. But this and similar matters *need not delay the construction of the telescope.*

6. Mr. A. speaks of only two plans for mounting, very properly rejecting Mr. P. Smyth's as too vast for us. But I think you proposed the German form, and I suggested Mr. Cooper's instrument as the model. At all events I do so now, and think that with the use of "Stirling's toughened iron" and short bearings, it will fulfil all necessary conditions. I may mention that Mr. Grubb has one with some improvements in the Dublin Exhibition, intended for a 12-inch achromatic of 20-feet focus. It seems very strong, and when you (and I hope others of the Committee) see it, you will be able to judge whether its type might not be preferable. Experiments on suspending the observer need not delay the preparation of the speculum.

7. The same may be said of photography; *it is not an essential part of the plan,* and experiments respecting it need not delay the progress of other matters; the effect of metallic reflexion in absorbing the chemical rays may be determined in a day But I must add that I expect nothing from photography for nebulæ; it may give the places of small stars, but this object can be as well attained by attaching a camera lucida to the eye-piece.

8. In a 4-feet Cassegrain the small mirror should be 9 inches diameter; you I think would make it 4 in a Newtonian. The difference of light is unimportant. I do not know why Mr. A. thinks the small mirror of the Cassegrain cannot be made of silver. A mere plating is alone required, and the curved figure is more easily obtained than the plane.

10. The end support is good and steady; but it remains to be proved that sufficient steadiness cannot be obtained by supporting the telescope at its centre of gravity, especially if that centre be brought (as it may by using a skeleton tube) very near the speculum.

As far as I can judge from description, in Mr. Lassell's telescope the centre of gravity is about 6 feet from the end of the box; and I have not heard that he complains of tremors. It is, however, under cover; but, on the other hand, I should expect the mounting to be as firm as Mr. Cooper's.

14. The edge-support of the speculum which I use, and in reference to which my remarks were made, is not a semicircle, but an entire flexible ring attached to the box by three screws, two of which are acting in any position. The action is very satisfactory with 15-inch mirrors. But if Mr. Lassell's second set of levers be adopted, the edge support presents no difficulty.

15. I have great hesitation in questioning any statement of Mr. A.'s about diffraction. Still I would submit to him whether the reasoning in his investigation on the rings (the only one on this subject which I have seen) fully bears out his conclusion, that the diffraction *of a system of radial bars* is inversely as the aperture. The cases are not quite analogous; but in your *divided* 3-feet speculum, the lines of separation provided every star above the 6th magnitude with a cross, and a single packthread across the box of the solid one gave Rigel two tails. You can, however, easily fix on the 3-feet such a system of bars and try the effect. But even were this objection shown to be invalid, I still prefer the German mounting to that proposed by Mr. A. as far simpler.

On the whole, then, I am more hopeful than Mr. A. The most important of the difficulties which he suggests can only be verified by trial; and, if they be examined fairly, will be found not insuperable if we once set to work. What are these difficulties in comparison of those which *you* had to encounter? Have we not the benefit of your experience, and may I hope also of your assistance? For I will not disguise from you that I think our best chance of success depends on your permitting us to cast and polish the speculum at Parsonstown under your eye; and to mount and try it there (a part of the scheme which all seem to think desirable) before sending it to its destination. The mounting can be constructed by any great English machinist, and easily forwarded thither; and if the Committee share this hope and you will consent, I am certain that we shall have complete success. Of course we should reckon on some preliminary trials as to the form of the telescope and the mounting of the speculum.

Believe me, yours ever,

T. R. Robinson.

The Right Honourable
The Earl of Rosse.

May 11, 1853.

Memorandum.—With respect to the concluding remarks of Dr. Robinson, I beg to observe that I should be most happy to afford the gentleman engaged in constructing the instrument all the assistance in my power in the *shape of information*, and also by lending him

any apparatus I might have suitable for his purpose, should he think it worth while to avail himself of it. I should not *on any account* wish to interfere in any other way in the construction of an instrument undertaken by the Government. Besides, I think it would be but fair that the gentleman who had undertaken the task should have the whole credit of it.

Rosse.

Extract of a Letter from Sir William Denison, Lieut.-Governor of Van Diemen's Land, to Colonel Sabine, dated Jan. 21, 1853.

"Capt. Kay has just sent me a copy of his orders from the Admiralty to break up the magnetic observatory at this place and return to England, unless indeed I would take the instruments and appoint an observer to carry on such observations as might be desirable:—now I am most anxious to maintain an observatory here, even for the moral effect it would produce, were there no special material benefits to be derived from it; but when I see that, in your address to the British Association, you laid great stress on the establishment of an observatory in the Southern Hemisphere, I have great hopes that I may reckon upon your support to a proposition which I have made to the Secretary of State to keep up and extend the present observatory. We want a fixed point with which to connect our triangulation, which may be carried across Bass's Strait and along the coast of New Holland so as to determine hereafter the length of an Arc of 20° of latitude and upwards: we want means of determining our time, and of rating chronometers for our shipping, in which the Colony has a great interest. I have the means of erecting cheaply all the buildings which might be required, and the cost of establishing an observatory would not be much. I have therefore accepted the offer of the Admiralty, and have appointed an assistant to take such magnetical observations as Capt. Kay may think desirable, and I trust that you will exert your influence to make the establishment as perfect as possible.

(Signed) "William Denison."